June 2, 1953

Dear Phil:

Your most recent latter (May 27) arrived yesterday, after I had sent off the airmail to your home address and the copies of our ms. I was relieved to see this, for your confirmation of the method of handling the ms. As you asked, I am returning the copy with your annotations.

The crossing of our correspondence can get to be perplexing; if this does not reach you before you embark for Lexington, I will not be toossorry, as the interval will help to straighten this out.

With SW-1031, I have been able to go a:b:a:b. The last two, especially, went very slowly. My technique for phase reversal always includes one, sometimes two, single colony isolations to avert any possibility of carryover of the previous phase. As you say, this may account for the slowness. Prior to the last step, I ran the a phase through motility agar again, and this may help. The very sluggish variation shown by these types is a serious impediment to more detailed study. I will be most interested to hear if you find some serums that are more effective.

I could also send you SW-1052 (c:b) and SW-1053 (c:a) from S. altendorf c —x SW-1031 a+b and —x a:b, respectively, and will be very happy to do so if you want to see these for yourself. But I am pretty sure these diagnoses are correct, and don't want to overload your assembly line with too many monsters.

With SW-1026, I got only i:b:— (or :z33) (like yourself). From N97 itself, the results have been variable. Some colonies were b:- (or :z33); others went b:1,2:—, and a few went b:1,2:b:— but no further. There is clearly some factor not under control. These colonies all came from a single colony from N97; it is evidently not a question of mixture in the old culture of N97.

I am a little shamefaced (not the first time) about 104305. Each of the swarms from S. gallinarum —x SW-1040 had been put directly in broth, and typed (as g...). The broths werecthen streaked out, and single colonies put into stabs to send you. In my haste, I did not recheck these single colonies, and G5 (as sent) is evidently a carryover of SW-1040 (cf. paragraph 3, sentence 3 above). I've set the experiment up again, and will reship when possible (after checking). There is not much point in doing any more with "G5".

In my letter over the weekend, I indicated my reservations about SW-1003 as a transduction. Your substantiation of V in this bug, and its concordance with abortus-equi in biochemical characters, are good reason for scrutinizing the genetic side of the story very closely. I was rather unhappy with SW726 owing to its very sluggish migration in motility agar, and its fairly poor over-all growth. From the cultures recently sent by yourself and Moran, I have picked one out (her #1966; my SW-1056) for closer study. I hope there is no question of this as a

regular ab.-equi. It swarmed the fastest, grew the best in broth, and gave the smoothest looking colonies of the group. I propose to make the following experimentak comparisons:

SW-1056 in enx serum for spontaneous changes

TM2 —x " " " "

TM copenhagen —x

PB odense —x

Group D
(dublin) —x

[SW-1056 is already giving several bulbs after 24 hours in enx serum. Part of the monophasicity of typical ab.-equi may have to do with sluggish migration even without serum inhibition. Is your experience different?]

to determine whether V comes up again, and if so, under what circumstances.

Would it be convenient for you to send me a spot of V serum (perhaps at 1/10 dilution). I could then do a preliminary screening, and not have to send so many cultures.

Since the SW-1003 result may be peculiar to #26, I will also try to study this more closely. One of Moran's strains ("Schofield") is essentially non-motile but x— TM2 gave numerous motile swarms. ~~Th-e~~ The same trick might work with #26.

I am having the pedigree sheets typed up in style.

TM2 is the culture used in the ab.-equi experiments. The culture sent you is probably a fresh single colony (for phase purity) from the original.

Hope you enjoy your trip.

Sincerely,

Joshua Lederberg